January 17, 1952

Dear Bruce:

Your friendly letter somewhat mitigated my disappointment at your indispensability to higher learning in Britain. I had no right to expect that you would be able to extend your visit, as Esther had told me you had discussed yourpplans with her. A shorter visit would be second best, and we approve heartily of the principle.

We have indeed worked full steam during the summers (except 1950 when I taught at Berkeley). Unfortunately, steam is almost too apt a description of the atmosphere, and we had more or less wanted to go somewhere else (anywhere would do) for part of this summer. However, our plans are by no means fixed, and my own academic responsibilities will be rather flexible, so that it may still be possible to work something out to our mutual advantage. Since the demands on you form somewhat of a fulcrum for any discussion, it might be simplest if you could first indicate your probable range of action in time, from which we could then try to work out the best arrangement. I was not entirely clear just when you could leave NYU, and the latest you had to be back in London. Three months would be about one-fourth as valuable as a year, and should be barely long enough to start and begin to finish something. I am delighted at the prospects of a resuscitation of H-phages: perhaps we could find something to do with them together. I am looking forward tosseeing what they really are

Since I wrote you, I've managed to shake off some of the incubus of writing, and am working on a very curious self-sterility situation in K-12. Esther had pikked it up some time ago, and a letter from Cavalli brought it again to my attention. It boils down to the fact that some strains are F-, most are F+, the symbols being definable by the condition that F- x F- is completely sterile, whereas F+ x F- and F+ x F+ are fully fertile. The inheritance of F was very peculiar, all sexual progeny being F+. Some experiments where a mixture of F+ and B- was used as "one" parent (nutritionally speaking) x F- led me to think for a while that an "F+ Hormone" was involved, but it turns out that the activation of F- by growth with F+ (in one experiment) was permanent, so that it looks like there is extracellular transmission of the F+ factor, almost certainly not via lambda. Filtrates of F+ cultures have not so far displayed any modification of F-, so that the experiments so far are not entirely satisfying. If this sounds confusing, don't blame yourself.

Sincerely,

Joshua Lederberg

Josh